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Earth System Dynamics
Discussions

Interactive comment on "Historical and future anthropogenic warming effects on the year 2015 droughts, fires and fire emissions of CO_2 and $PM_{2.5}$ in equatorial Asia" *by* Hideo Shiogama et al.

Anonymous Referee #3

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General comments: This study examines global warming impacts on fire activities, focusing on the burned area and fire emissions of CO2 and PM2.5 over equatorial Asia. Considering June-November 2015 when a strong El-Nino induced a large decrease in precipitation over the area, the authors examine changes in the probabilities of droughts and fire activities due to anthropogenic influences using the MIROC5 AGCM large ensemble (100 members) simulations. They find increased probabilities of the droughts and fire activities as global warming become stronger. In particular, they show that 3.0 degree warming that represents the current mitigation policies would bring severe droughts and increased fire activities due to the intensified El Nino at near 100% chance. I find this paper overall well written, providing interesting and policy-

relevant results. However, there are a few issues, mostly related to uncertainty factors, which need to be improved through revision.

Major points: 1. Model dependency: It would be useful to discuss limitations of the atmospheric model experiments and its possible impacts on the results. Particularly, precipitation changes in the future warming simulations are shown to be critical for determining changes in fire burned area and CO2 and PM2.5 emissions (Fig. 8), but atmospheric models tend to have large biases in precipitation over the Tropics partly related to the omission of air-sea coupling. It seems that normalized precipitation is used to overcome this problem but some justification would be needed with showing precipitation bias of the model. In addition, future projections of precipitation look highly dependent on the SST change patterns (Fig. 4). Uncertainty in these SST change patterns needs to be discussed as well.

2. New findings: New results compared to previous studies are not clearly explained, in particular, in view of Lestari et al. (2014). What advances have been achieved by increasing ensemble size? Adding more information on this would be helpful such as how to construct ensembles and how uncertainty is assessed with the large ensemble simulations. Also, the empirical relation between precipitation and fire activities is used to estimate future changes in fire activities and the authors consider its uncertainty somehow in their analysis. I think this part is important and more details needs be provided on its uncertainty ranges and associated impacts on main results. See my specific points below.

3. Implications: The last part on implications is rather confusing and hard to follow. I would suggest rephrasing it for better understanding. For example, it is unclear what are exactly compared between MIROC5-based estimations and diverse SSP scenarios: fire CO2 emissions due to climate change versus land use CO2 emissions? From this comparison, the authors seem to suggest that additional fire CO2 emissions due to climate change should be considered in SSP scenarios, but this interpretation is not that clear at the present form. I am wondering if it can be made more specific by

suggesting how much increase in CO2 emissions should be added, for example.

Specific points: Title: “year 2015” sounds a bit strange to be connected with “future” warming effects. How about saying “2015-like” or similar instead.

L87-88: How is the EA box selected? I think it can be adjusted (e.g., narrower in zonal direction) to better capture the P decrease area. Or it doesn’t matter since only land is considered? Please clarify this.

L91-92: In line with “precipitation anomalies and accumulated water deficits”, wouldn’t it be better to use accumulated precipitation like SPI?

L93, L108: “divided by standard deviation”. Can we assume normality for precipitation and omega anomalies? Area averaged 6-month mean values might be okay but a quick check would be useful.

L115: I would suggest providing more details on how “long-term anthropogenic signals were removed” as SST patterns are important for determining precipitation responses to El Nino.

L122: “100 member ensembles during 2006-2015 with 1.5 degree and 2.0 degree warming”. Do it mean that ensemble runs are performed only for Plus15 and Plus20 or there are 100-member HIST runs for 2006-2015 as well?

L141-144: This way of sampling looks important to capture uncertainty arising from internal variability, and showing resulting spreads in P and omega responses in Fig. 7 would be interesting. Also, it would be useful to explain here how to construct CDF using 1000 samples and estimate probabilities exceeding the observed value and its 10-90% confidence intervals.

L150-153: Why stronger El Nino (and P responses) are simulated in 1.5 degree warming simulations than 2.0 degree ones? Some discussion needs to be provided. Does it occur in all 1000 samples? Do other HAPPI models share this or is this a characteristic of MIROC5?

L161: “1000 random samples of the regression factors in Eq. 1”. Please provide details given its importance. Also see my major comment.

L163-164: Please explain how to assess significance of this change.

L169: Why is the emission of Japan used as reference here?

L172: First sentence. This needs to be mentioned clearly above and also in figure captions to avoid confusing.

L172-188: This paragraph and Fig. 9 are hard to follow with many skips and limited explanations. Please consider rephrasing it. See my major comment above.

L196: “82%, 68%, and 93%”. Please add uncertainty ranges or indicate these are ensemble means or medians. Same for L204.

L199-202: Model dependency issue is here. How representative is MIROC5 projected precipitation in the future? Any comparison with other models would be useful. See my major comment above.

L205: “additional changes”. Are these significant?

L209: “modifying fire CO2 emissions scenarios”. Can authors suggest how much modification is needed? See my major comment above.

Fig. 1: Line 342: “left panels” should be “right panels”.

Fig. 2: There seems to be a stronger case than 2015, perhaps 1998? Where is 1982 that has also a stronger P decrease in Fig. 3? It may affect fitted curves.

Fig. 3: Indicating 2015 case in time series and scatter plots would be useful. Is there any underestimation or overestimation by models in P and omega responses?

Fig. 6: It’s not clear why difference from NAT is shown even for future changes. Is this for 2015 or using all years?

Fig. 7: Is this also for 2015? Related to my major comment on model dependency

issue, are these are supported by other coupled models?

Fig. 9: Difficult to understand. How is the CDF of CO2 emissions (red curves) estimated? Are these CO2 emissions only due to increased fire over equatorial Asia?

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